Perfect

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Admiral MANNERS, President, in the Chair.

Sir William Thomson, Glasgow University;
Rev. E. Crofton, Oak Lawn, Wootton Bridge, Isle of Wight;

were balloted for and duly elected Fellows of the Society.

The Society have received from the Hydrographic Department of the Admiralty Observations of the Total Eclipse of 18th August, 1868, made by Staff-Commander Reed, commanding H. M. Surveying Vessel *Rifleman*, and Mr. Pope Hennessey, Governor of Labuan, at Barram Point, on the N.E. Coast of Borneo. Mr. Pope Hennessey's Observations have been published in the Proceedings of the Royal Society.

The Society have also received from the Peninsular and Oriental Company accounts of the Eclipse, as seen by their Commanders — Captain H. W. King, Latitude 16° 44′ N., Longitude 83° 55′ E., within the totality — Captain C. G. Perrins, commanding S. S. *Carnatic*, Latitude 16° N., Longitude 54° 15′ E., 20 miles north of the limit of totality on that meridian — Captain D. Rennoldson, commanding S. S. *Rangoon*, Latitude 15° 42′ N., Longitude 59° 15′ E., on the central line — and Captain J. S. Murray, then in the Formosa Channel, considerably north of the limit of totality.

Observations of the Solar Eclipse, August 18th, 1868.

By J. Tebbutt, Esq.

The solar eclipse of the 18th Aug. was pretty well observed here. Although the Sun was occasionally hidden from view by passing clouds, the beginning and the greatest obscuration were well seen. A remarkable phenomenon presented itself about half a minute before the instant of first contact. A dark streak or band became visible for several degrees along that limb of the Sun to which the Moon was approaching, and after continuing visible for a few seconds, vanished. It reappeared, however, for a moment immediately before the contact. The darkness seemed to extend along the narrow interval separating the limbs of the Sun and Moon, but did not encroach on the solar disk. This phenomenon was so palpable that it served as an excellent warning of the time of contact. A very slight flattening of the Sun's limb was also remarked just previous to the contact, but this was quite distinct from the indentation produced by the Moon. The Sun was shining beautifully clear at the time. The first contact was pretty accurately observed at $4^h\ 6^m\ 30^s$, local mean time, the telescope employed being my $3\frac{1}{4}$-in. refractor with a power of about 30. During the progress of the eclipse there was a slight boiling of the limb both of the Sun and Moon. This, however, was the result of atmospheric causes, and was not so great as I expected from the low altitude of the objects. With the exception of the slight disturbances from this cause, the Sun's cusps were sharp and well defined. A careful scrutiny satisfied me that not the slightest portion of the Moon's limb could be even faintly distinguished beyond the cusps. No remarkable irregularities were observed on the Moon's limb. Two very fine spots, each surrounded with an extensive penumbra, and attended with several smaller spots, were very conspicuous on the southern half of the Sun's disk, but were not eclipsed by the Moon. A magnificent tract of faculæ, situated near the larger spot, contrasted very beautifully in colour with the general ground of the solar disk. About one-third of the Sun's diameter was eclipsed at the time of greatest obscuration. The Sun disappeared behind a dense cloud on the horizon at $5^h\ 16^m$ P.M.

Meteorological observations were taken through the day, but nothing remarkable, beyond the unusual steadiness of the barometer, was recorded. The observations of the black-bulb thermometers were much interfered with by passing clouds.

Windsor, New South Wales,
September 1st, 1868.

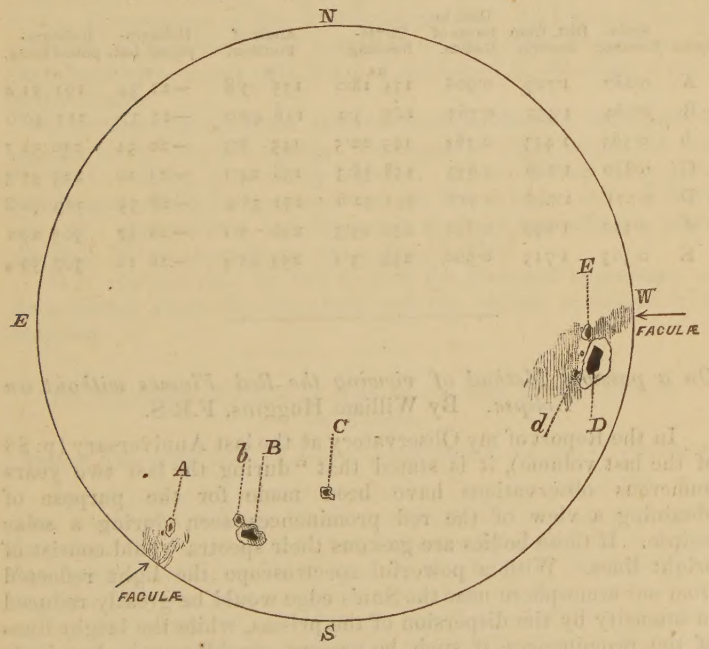
Sun-spots and general Aspect of the Sun on the day of the Total Eclipse, August 18th, 1868.

(*Communicated by Messrs. De la Rue, Stewart, and Loewy.*)

The history of the great eclipse would, in our opinion, be incomplete without a record of the heliographical positions of the Sun-spots observed at the time.

The unfavourable weather in England on the days before and after the eclipse prevented photographic observations, but we were so fortunate as to obtain a sun-picture on the day of the eclipse, at $3^h\ 29^m$ G.M.T., about eight hours after the phenomenon.

There were two comparatively large groups of spots on the Sun, one near the eastern, the other near the western, limb. Both groups were connected, as shown in the accompanying rough



Rough Sketch of Aspect of Sun.

sketch, with faculous matter which apparently extended beyond the visible disk on both sides. The faculæ consisted chiefly of very numerous small scattered patches, here and there running together and forming larger flocculi; they covered altogether an unusually large area, indicating the extraordinary activity which

was also observed by Dr. Janssen, during and after the time of the eclipse, on the surface of the Sun.

The angles of position of those two parts, where the faculæ seemed to extend into the invisible hemisphere, may roughly be stated as 145° for one patch, and 255° for the other, reckoning from north towards east. How far these positions agree with any observed protuberances can only be gathered after some time, when the whole of the detailed accounts have come to hand.

The heliographical elements of the Sun-spots, forming the two groups, were calculated in accordance with the principles explained in a paper read before the Royal Society some time ago, which we hope to see soon in the hands of the printer. These elements are given in the following table, where the letters for each spot correspond with those on the accompanying sketch.

Heliographical Elements of the Sun-spots, observed at the Kew Observatory, on August 18, 1868, $3^h\ 29^m$ *G.M.T.*

Spots.	Scale-Reading.	Dist. from Centre.	Dist. in terms of Radius.	Circle-Reading.	Angle of Position.	Heliographical Lat.	Heliographical Long.
				° ′	° ′	° ′	° ′
A	0·287	1·729	0·908	135 18·0	135 3·8	−21 34	191 51·2
B	0·564	1·452	0·763	147 3·2	146 49·0	−23 37	211 40·0
b	0·563	1·453	0·763	145 22·5	145 8·3	−22 34	210 52·7
C	0·810	1·206	0·633	158 38·3	158 24·1	−23 10	225 57·3
D	0·271	1·745	0·917	251 52·6	251 38·4	−28 55	309 30·8
d	0·317	1·699	0·892	250 23·3	250 9·1	−28 47	305 29·1
E	0·303	1·713	0·900	254 3·1	253 48·9	−26 12	307 59·4

On a possible Method of viewing the Red Flames without an Eclipse. By William Huggins, F.R.S.

In the Report of my Observatory at the last Anniversary (p. 88 of the last volume), it is stated that "during the last two years numerous observations have been made for the purpose of obtaining a view of the red prominences seen during a solar eclipse. If these bodies are gaseous their spectra would consist of bright lines. With a powerful spectroscope the light reflected from our atmosphere near the Sun's edge would be greatly reduced in intensity by the dispersion of the prisms, while the bright lines of the prominences, if such be present, would remain but little diminished in brilliancy. This principle has been carried out by various forms of prismatic apparatus, and also by other contrivances, but hitherto without success." The observations of the eclipse of August last having shown the position in the spectrum of the bright lines of the red flames, Mr. Lockyer and Mr. Janssen succeeded independently, by a similar method, in viewing the spectra of these objects.

My object in this Note is to describe one of the "other contrivances" mentioned in the Report.

The apparatus consisted of screens of coloured glasses and other absorptive media, by which I was able to isolate portions of the spectrum. It appeared highly probable that if the parts of the spectrum which then alone remained were identical with those in which the bright lines of the flames occur, these objects would become visible.

For this inquiry I obtained a great variety of coloured glasses and other absorptive media. I first examined them with a prism to learn the absorptive power which they exercised on different parts of the spectrum. I then combined them in various ways. These glasses were sometimes employed before the eye, but more frequently by projecting the image of the Sun's edge upon a screen, after the light had been sifted by the coloured media. In making these experiments means were taken that the whole of the Sun's image should be got rid of, in order that the eye, kept in comparative darkness, might be more sensitive to the greatly feebler illumination of the objects sought for. As I had no knowledge of the position in the spectrum of the bright lines, it would have been by accident only if I had succeeded in obtaining a view of the flames.

Now that the positions of these lines are known, this method appears to be very promising. Perhaps the light about the red line at C will be most easily isolated. I have a deep ruby glass which cuts off all the spectrum except the extreme red. I have since the observations only been able to make one attempt, when the state of the atmosphere was unfavourable.

It is obvious that by this method the form and appearance of these flames could be observed, and the objects measured with accuracy.

On a possible Method of viewing the Red Flames without an Eclipse.

(*Extract from Letter from Sir John Herschel, Bart. to the Rev. C. Pritchard.*)

I send you an extract from a letter of my son, Lieut. Herschel, from which you will see that immediately on his ascertaining the fact that the light of the protuberances exhibited three *bright* lines corresponding to *dark* lines in the light of the Sun's photosphere, he was at once led to speculate on the possibility of taking advantage of this fact to obtain a view of the protuberances apart from the condition of a total eclipse. That such *glasses* as he contemplates (or other absorptive media) could be found, there is, I fear, little hope; and my only view in calling attention to this suggestion is to show how immediately and readily a clearly defined new fact of this sort suggests to an active and combining intellect the possibility of immediate practical application. If I mistake not, the "double discovery" of Mr. Lockyer and Mr. Janssen, of a mode of rendering the pro-

tuberances *sensible* (not visible) by spectroscopic analysis of the light close to, *but beyond* the photosphere in ordinary daylight, turns upon the very same feature — viz. the *absence* of the three rays D, &c. in the photospheric light (and therefore in the *general* skylight) and its *presence* in that portion of the skylight which is superposed on the prominences; so that, while skylight *in general* exhibits necessarily three *black* lines corresponding to D, &c. — at that particular spot where a prominence exists, they should not be exhibited at all, or very feebly.

Extract of a Letter from Lieut. J. Herschel, dated Bangalore, Sept. 2, 1868.

"It has occurred to me that, since the whole light of the 'flames' is of three refrangibilities only (or nearly so), dark glasses (could they be formed) which allowed these *only* to pass, would so enormously diminish the light *from the Solar disk*, as to enable the flames to be seen without the interposition of an opaque body, *i.e.* without an eclipse. When I say dark glasses, I mean, of course, *any* media which have the effect required. Is the idea a practical one? If it is, these flames may come to be studied *at leisure!*

"I will not forget *ἡ Argûs* you may depend."

Collingwood, Nov. 3, 1868.

Remarks on Mr. Stone's Rediscussion of the Transit of Venus, 1769. By S. Newcomb, Esq.

I beg leave to offer to the Royal Astronomical Society the following remarks on Mr. Stone's very interesting re-discussion of the observations of the transit of *Venus* in 1769, which appears in the October number of the *Monthly Notices.*

Mr. Stone's interpretation of Chappe's observation of first contact at egress seems to me untenable. To understand the question at issue it must be borne in mind that, owing to irradiation, after the planet has apparently entered upon the solar disk, it is still connected with the latter by a dark protuberance, or "black drop," which rapidly grows thinner, and at last breaks off, leaving the planet at a sensible distance from the edge of the disk. This phenomenon marks the true time of external contact, and is that to which the attention of the observer should always be directed. At the second external contact the same phenomenon may be observed in a reverse order, a black ligament suddenly forming between the planet and the edge of the solar disk.

The question is whether, at this contact, Chappe observed the sudden formation of the ligament, or whether he waited for the moment when the apparent limbs of the two bodies would have touched each other if the drop had not been there. Chappe says (I quote from Mr. Stone):

"Temps vrai $5^h\ 54^m\ 50^s$. Premier contact à la sortie. * * * A ce premier contact *Vénus* s'est allongée plus considérablement que le matin en s'approchant tout-à-coup du bord du Soleil."

If, as Mr. Stone assumes, the time given in the first sentence of this extract does not refer to the phenomenon described in the last sentence, it must be either (1) that by the words "ce premier contact" Chappe meant something different from the "premier contact" he had observed, or that by the preposition "à" he meant some time before. His language does not seem to me to admit of either of these meanings. Moreover, from his account of the ingress it appears that Chappe was acquainted with the phenomena, and knew what was to be observed. "Je n'observai point pour l'instant de l'entrée total celui ou le bord de *Vénus* commençoit à s'allonger, mais ne pouvant pas douter que ce point noir ne fît partie du corps opaque de *Vénus*, j'observai le moment où il étoit à sa fin."

Mr. Stone's view does not seem at all strengthened by the observation of Pauly, for Chappe says he saw *Venus* perfectly after Pauly had left his telescope, and intimates that the reason of his leaving so soon was his want of practice in observation. But if Mr. Stone's view is right, and if Chappe saw the phenomenon he describes, Chappe must have seen the very thing he wanted to observe at the moment Pauly left the telescope. His last remark would then hardly be consistent with honesty.

A re-solution of Mr. Stone's equations on the hypothesis here maintained leads to the following results:—

Normal Equations.

$$16x - 64{\cdot}4\,\delta\pi = +358{\cdot}0$$
$$21y - 110{\cdot}2\,\delta\pi = -56{\cdot}4$$
$$-64{\cdot}4\,x - 110{\cdot}2\,y + 123134\,\delta\pi = +45536.$$

Solution.

$$x = +24^s{\cdot}0 \qquad y = -0^s{\cdot}6$$
$$\delta\pi = +0''{\cdot}38 \qquad \pi = 8''{\cdot}87$$

If the equations in x and y are treated separately, the results will be—

From equations in x, $\pi = 8''{\cdot}91$
From equations in y, $\pi = 8''{\cdot}70$.

A Reply to Mr. Newcomb's Remarks on "Mr. Stone's Rediscussion of the Transit of Venus, 1769." By E. J. Stone, Esq.

I would remark, in the first place, that Mr. Newcomb has not touched any point of primary importance in this discussion. The real issue between the old value, of 8″·58, and mine, of 8″·91, is no less than 0″·33. The point Mr. Newcomb has raised is a question of only 0″·04, viz. between my value and 8″·87. The question is, therefore, one of comparative insignificance, but the observations will not, I believe, bear even this slight change.

Mr. Newcomb's point is, that I have erroneously assumed that Chappe, at the egress, observed an apparent internal contact instead of the real one or formation of the black drop. His only argument is that it would be absurd to suppose that he would do so. I should have thought that my meaning was sufficiently clearly expressed. My words were, "I consider, from the description given by Chappe himself, that at the egress he did not catch the formation of the black drop. He appears to have been surprised by the contact being established all at once." Take these words in conjunction with Chappe's description of his observation, "Le soleil étoit ondoyant ainsi que *Vénus*, ce qui rendoit cette observation très difficile. A ce premier contact *Venus* s'est alongée plus considérablement que le matin, en s'approchant tout-à-coup du bord du Soleil." It appears to me you have exactly my meaning, that although Chappe fully intended to observe the same phenomenon at the egress which he observed at the ingress, yet owing to tremor and the difficulty of the observation he did not catch the first formation of the drop, but some phase much nearer to the apparent internal contact. Calling to mind Wales' observation of 24ˢ between the real and apparent contacts at the egress, I maintain, in spite of Mr. Newcomb's remarks, that Pauly's positive observation of same contact 22ˢ before the time given by Chappe does strengthen most materially the inference which I have drawn from Chappe's own words.

It is an unfortunate fact for Mr Newcomb's argument about the "premier contact," that at the ingress Chappe describes the phenomenon observed not as a "contact" but as "l'entreé totale." Mr. Newcomb appears to me to have mixed up his own inferences and Chappe's words respecting Pauly and his observation. Pauly appears to have occupied a position equivalent to that of an English Royal Engineer. Chappe says that he was an observer of a little, *i.e.* some experience. An entirely different impression from that given by Mr. Newcomb's words.

Again, when Mr. Newcomb says that "Mr. Stone's view does not seem at all strengthened by the observation of Pauly, for Chappe says he saw *Venus* perfectly after Pauly had left his telescope," Mr. Newcomb has overlooked the fact that this disappearance of *Venus* refers to the *total egress* of the planet from

the disk, and therefore proves nothing with respect to Pauly's observation of internal contact about 19^m before.

The observations appear to have been arranged as follows: An assistant was stationed near the clock to give out the time, the observers themselves only comparing their time with the clock directly as a check. There is no doubt but that Chappe, with his more powerful instrument and the extraordinary care and precautions he took to catch the last glimpse of the planet on the Sun, did see *Venus* long after Pauly had lost sight of the planet. Chappe distinctly states that without the precautions he adopted to secure this last glimpse an enormous error would have been made. This, however, does not prove that Pauly's positive evidence of a contact, an entirely different and much easier observation, should be entitled to *no weight.* Chappe undoubtedly thought that Pauly's observation was too early, because he had not himself caught it. I consider this accounted for by the difficulty of the observation owing to tremor.

I am quite at a loss to find what evidence Mr. Newcomb has collected that Pauly left the telescope after the internal contact at egress, and that Chappe, his attention being thus called to the point, looked particularly and could then see no trace of the contact being established. There is no such statement in Chappe's journal, and Chappe never lived to return. Mr. Newcomb has blended together Chappe's remarks relating to the total egress, or final disappearance of the planet from the Sun's limb, and applied them by construction to the observation of internal contacts. If Chappe has made no remarks about Pauly leaving the telescope near the time of internal contact, Mr. Newcomb's dilemma has no force. I have, perhaps, dwelt too long upon this point, but it is the only one raised by Mr. Newcomb. There is a difficulty respecting these St. Joseph observations which has apparently escaped Mr. Newcomb's notice. There were three observers near St. Joseph, Chappe, V. Doz, and Medina. The observed durations agree so closely that whatever assumption be made respecting the one must be made respecting all. It certainly appeared to me so arbitrary an assumption that all three observers had missed the first formation of the black drop at the egress that, in spite of Chappe's language and Pauly's observation, I at first assumed that real contacts must have been observed. The value of solar parallax deduced from the equations on these suppositions was perfectly satisfactory to me, but the residual far exceeded any probable errors and the difference in time between the two phases was not satisfactory. I then, on re-consideration, came to the conclusion, which I still think satisfactory, that if Chappe's words indicate that he lost the first appearance of the black drop, the same atmospheric disturbances, the same boiling of the limbs of the Sun and *Venus*, similar difficulties of observation, in fact, might equally prevent the first appearance being caught by V. Doz and Medina, who were observing near. My argument is this,—We have two distinct phases of a pheno-

menon, the real and apparent contacts, separated roughly by about 20^s: a quantity too large to be confounded with mere error of observation. If an observer states positively that he has observed a real contact, I have no right to assume that he has observed an apparent contact. If the observer says that he has observed an apparent contact, I have no right to assume that he has observed a real contact; but if the observer's words are doubtful, and I find that, by assuming he has observed an apparent contact, all the observations are rendered accordant, whilst by assuming that he has observed a real contact, all the observations are rendered discordant, then I maintain that it is not only permissible, but necessary, that we should assume that apparent contacts were observed.

The appeal in such a case must be to the residual errors. I have here placed my argument on its lowest ground. I believe that Chappe's own words require the assumption which I have made.

The following are the residual errors in my solution and in the modified form proposed by Mr. Newcomb.

Observer.	Stone's *solution* s	Newcomb's *modification*	Mean for S.	Mean for N.
Hell	− 1·7	− 0·8		
Sajnovics	+ 2·8	− 5·3	+ 0·6	− 3·1
Wales	+ 0·4	+ 7·9		
Dymond	− 0·6	+ 8·9	− 0·1	+ 8·4
Rumovsky	− 0·9	+ 6·9	− 0·9	+ 6·9
Chappe	+ 0·5	− 6·6		
V. Doz	− 0·4	− 5·7	− 1·6	− 4·3
Medina	− 5·4	− 0·7		
Green	+ 5·8	− 1·8		
Cook	− 4·2	+ 8·2	+ 0·8	+ 3·2

Mr. Newcomb's modification has increased the sums of the squares of the residuals at the five stations nearly 40 times.

It was these residuals, the unsatisfactory value of $\frac{x-y}{2}$, and Chappe's own language concerning the difficulty of the observation at egress, the establishment of the contact all at once, and Pauly's positive observation of a contact 22^s before Chappe's observation, that drove me to assume that Chappe did not, as a matter of fact, catch the first formation of the drop, but some phase much nearer the apparent contact. Mr. Newcomb has only reproduced one of the many slight variations I have tried upon this question.

The following table will perhaps show how well this transit of *Venus* of 1769 was observed; how favourable, on the whole,

were the positions of the observers; how impassable a barrier any satisfactory discussion of these ten observed durations places between such values of the solar parallax as 8″·58 and 8″·91.

If the value 8″·58 of the solar parallax satisfied the ten observed durations, we should have to assume the following errors to have been made in the mean of the observed durations at each station, in order that 8″·91 could be admitted as the true value of the solar parallax.

+ means that the observed duration is too small.

Station.	No. of Obs. durs.	Greatest discordance.	Assumed Mean Error.	Stone's Residuals.
		s	s	s
Wardhus	2	4·5	+24·9	+0·6
Kola	1	..	+25·3	−0·9
Hudson's Bay	2	1·0	+ 7·8	−0·1
St. Joseph	3	5·9	−10·3	−1·6
Otaheite	2	10·0	−27·5	+0·8

Such errors, as the results of observation, are simply impossible, and if a satisfactory solution of these ten durations had ever been obtained with a value of the solar parallax 8″·58, then, whatever might have been the results derived from our other methods, the true value of the solar parallax could not have been 8″·91, nor any value near it. The assumed mean errors show most clearly the grounds upon which astronomers refused to give up the value 8″·58. The error was impossible, only provided the discussion of the results had been satisfactory. The solutions obtained, however, with the value 8″·58 have never satisfied the observed durations. Exaggerating the importance of some alterations in Hell's Journal, a solution has been forced by virtually assuming the observations at Wardhus to have been forgeries. Even this, however, has not been sufficient, the Kola observations still stood out as a warning. In my solution I have rejected nothing for discordance. The residuals are all reduced, in the mean results, within 1ˢ·6, a quantity within probable errors of observation. I consider, therefore, that this great problem of the determination of the mean distance of the Sun from the Earth, may be considered as almost settled within the requirements of our present astronomy. All our work is now accordant, and points to a value about ninety-one millions seven hundred thousand miles.

Observations made at the Royal Observatory, Greenwich, of the Egress of the Planet Mercury from the Sun's Disk, 1868, *Nov.* 4.

(*Communicated by the Astronomer Royal.*)

Observations by E. J. Stone.

The instrument employed was the Great Equatoreal. Aperture of object-glass, 12¾ inches.

My attention was chiefly directed to any peculiarities which might be presented during the transit, and not to time-determinations.

With power 137, a ring of light was clearly visible around the disk of *Mercury*. It extended to a distance of nearly a semi-diameter.

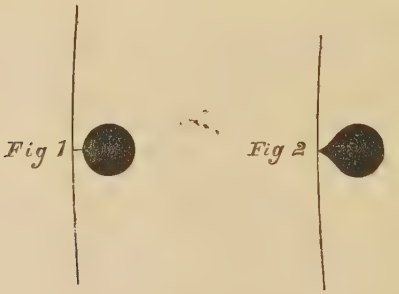
The ring was still visible with a power of 245, and did not disappear with any changes in the coloured glasses employed. I am of opinion that it arose from mere contrast.

The central part of the disk was certainly blacker than the edges.

I could see no central bright spot.

I could see no satellite.

At the first contact, without the slightest deviation from circular form, a very fine ligament was suddenly seen, connecting the planet and the Sun's apparent limb (fig. 1). This ligament



very quickly became broader, and made it extremely difficult to estimate the time of apparent internal contact, which, however, I estimated at about fifteen seconds after the first phenomenon of the connecting ligament. On looking at the half-second chronometer after the formation of the ligament, the transit-clock time was $11^h\ 59^m\ 14^s{\cdot}5$, but this was some seconds after the first formation of the ligament. The corresponding G.M.T. would be $21^h\ 0^m\ 5^s{\cdot}5$.

Observations by Edwin Dunkin.

The observations were made with the Altazimuth; power about 100; clear aperture of object-glass $3\frac{3}{4}$ inches:

	h	m	s	
First contact	21	0	6·2	G.M.T.
Last contact	22	2	35·7	

The time recorded at the first contact was when the limb of the planet was first seen in coincidence with the Sun's limb, at the moment when it was noticed suddenly to assume a balloon or pear shape. I could not see any very narrow filament between

Mercury and the Sun's limb before the recorded time. The apparent internal contact of the limbs must have been about 10^s after the recorded time. At the first contact the appearance was similar to that represented in Fig. 2.

Mercury was densely black in the interior parts of the planet, but near the limbs I thought that there were appearances of a slight illumination. The limbs had a hazy appearance. No bright spots seen on *Mercury*. The observed times were registered on the chronograph.

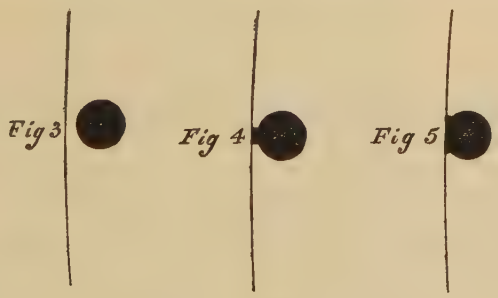
Observations by G. S. Criswick.

The observations were made near the thermometer stand on the magnetic ground, with the 46-inch telescope, and power of 170. The times were observed with a mean solar chronometer (Carter 154) which was compared by means of coincidences with the transit-clock directly afterwards.

	h	m	s	
First contact	21	0	12·8	G.M.T.
Last contact	21	2	42·8	

The limb of *Mercury* seemed very rough when seen on the Sun's disk.

Just before the planet arrived at the Sun's limb, it became somewhat elliptical (fig. 3), and then suddenly sent out a black



point to the Sun's limb (fig. 4): this was the phenomenon observed for "first contact:" the time given is perhaps two or three seconds late, as I was not counting the chronometer at the time. The observation of the last contact is considered pretty accurate.

The distortion of *Mercury* made the time of "bisection" too uncertain to be worth observing.

Observations by W. T. Lynn.

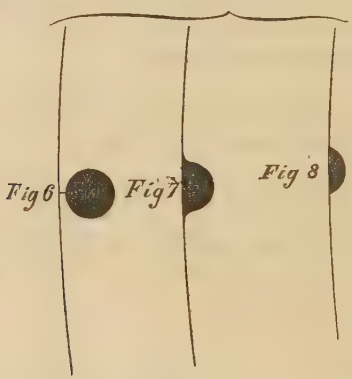
The observations were made with the North Equatoreal, power about 170.

	h	m	s	
First contact of filament	21	0	0·7	G.M.T.
Contact of limbs established by	21	0	32·6	
Bisection	21	1	31·5	
Last contact	21	2	37·3	

The planet as it approached the Sun's limb appeared, as it were, elliptical on the side nearer the Sun's centre.

The first time noted is when a thread or filament of blackness seemed to issue from the planet and touch the Sun's limb (fig. 6). It is considered pretty exact.

The time of contact of limbs is doubtful, the planet assuming the shape shown in fig. 7. The time is, therefore, an estimation that they were certainly in contact by at least that time.



The time of bisection is probably true to within about 2 or 3 seconds (see fig. 8).

The last contact is, I believe, true within a second, as I certainly saw an indication of the planet a second before the time noted, at which time it had totally disappeared.

The Sun's limb was well defined, and the tremor by no means considerable.

Observations by J. Carpenter.

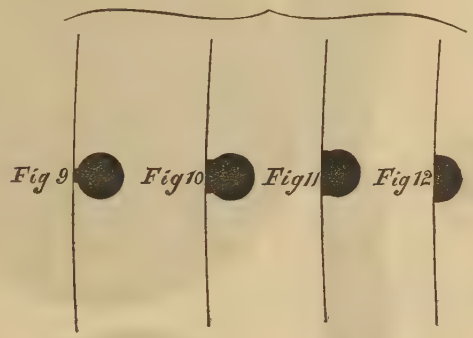
The telescope used was one of the former collimators of the Transit Circle; $3\frac{3}{4}$-in. aperture, 62-in. focus, power 90.

	h	m	s	
First contact	21	0	14·1	G.M.T.
Last contact	21	2	40·1	

At the first contact the disk of *Mercury* was distorted as shown in fig. 9.

The black ligament appeared to form instantly and to be of the breadth indicated, about $\frac{1}{3}$ the diameter of the planet.

The same character of distortion remained till the planet crossed the edge of the Sun; the successive phases being as shown in figs. 10, 11, 12.



The squareness of outline of the black ligament was very striking; its appearance was not altered by varying the density of the glass shade. A graduated shade was used.

Observations by H. J. Carpenter.

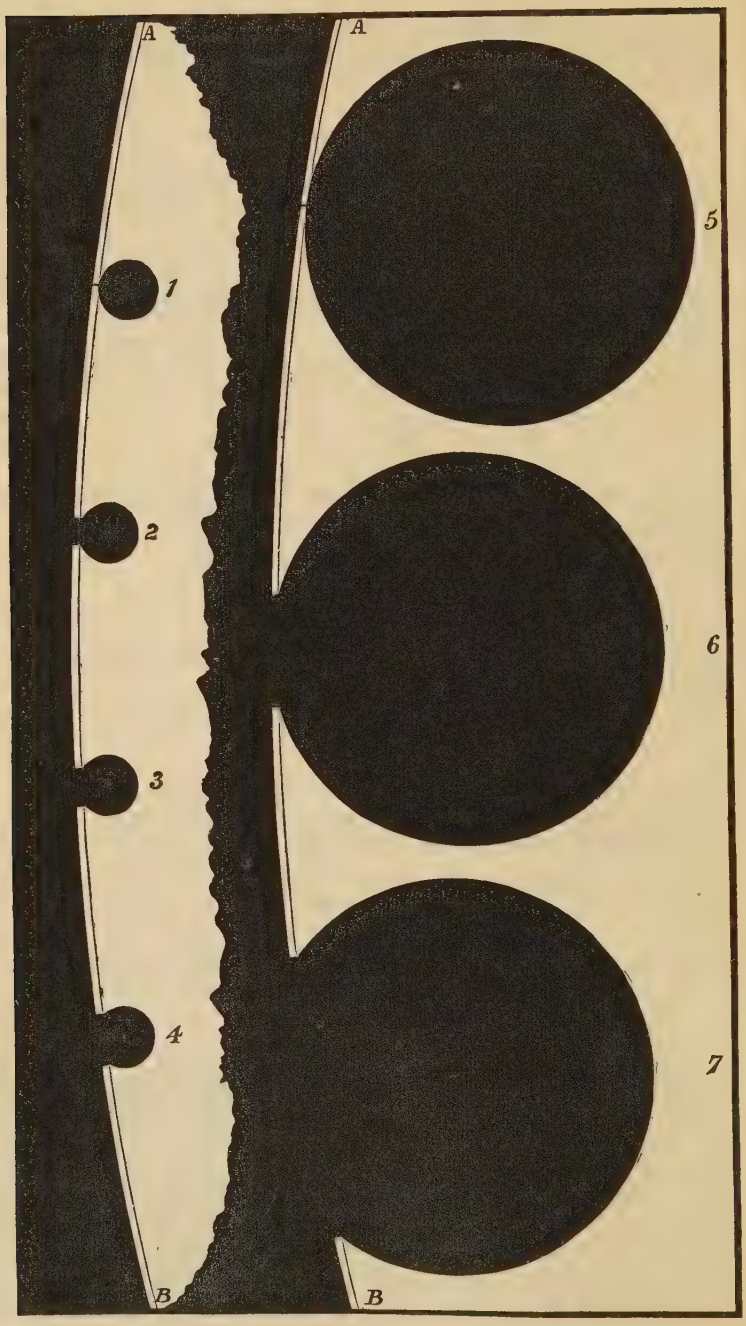
The observations were made with the East Equatoreal; power 70.

	h	m	s	
First contact	12	0	11·1	G.M.T.
Last contact	12	2	36·7	

The time noted for first contact is that of internal contact of the limbs when light first ceased to be seen between them. That of last contact is considered pretty exact.

Some Remarks and Suggestions arising from the Observations of the Transit of Mercury across the Sun's Disk, Nov. 4, 1868. By E. J. Stone, Esq.

The diagram with this paper is intended to represent the henomena presented at the ingress and egress of *Mercury* and *Venus* across the Sun's disk. I have allowed about $\frac{1}{60}$th part of the diameter of *Venus* for the effects of irradiation. The extreme boundary of light and shade represents the apparent limb of the Sun. The true limb of the Sun may be considered to fall half way between A B, and the boundary of light and shade. When the true limb of the planet reaches the true limb of the Sun, or the apparent limb reaches A B, the light is cut off, and a fine dark ligament appears to connect the limbs of the planet and the


A
A
1
2
3
4
5
6
7
B
B

Sun. This is the true contact: the time of the formation or breaking of the so-called black drop.

Figs. 1 and 5 represent this phase respectively for *Mercury* and *Venus*.

Figs. 3 and 6. The appearances presented at apparent internal contact.

Figs. 2, 4, and 7. Appearances at intermediate phases.

Fig. (7) is intended to represent the phase of figs. 2 and 4 of Cook's and Green's observations in 1769, the point of comparison being the breadth of the ligament.

With respect to the Greenwich observers, the first appearance of the contact appeared to me to take place without the slightest previous distortion of the form of *Mercury*, the ligament appearing almost like a cobweb for fineness.

Mr. Lynn appears to have first caught a phase very near that of fig. (1). Mr. Dunkin, a phase between figs. (2) and (3). Messrs. Criswick, James Carpenter, and H. Carpenter, phases somewhere between (2) and (3).

Mr. H. Carpenter has not given any drawing. He stated, however, that he tried to catch the time when the thread of light entirely disappeared. On my requesting him to point out which of the figures appeared to him to represent most nearly the appearance of the planet at the time of his observation, he, without hesitation, pointed out fig. (3). His time agrees closely with the time given by Messrs. Criswick and James Carpenter. I would particularly call attention to the differences in aperture and optical power of the instruments employed by the different observers.

It appeared to all of the observers difficult to catch the time of apparent internal contact; figs. (3) and (6) are intended to show the comparative facility with which this phase can be seized in the case of *Venus*.

As practical hints for guidance in future observations of a Transit of *Venus*, I would suggest for consideration the following points:

1. That telescopes of nearly the same aperture should be employed.
2. That very nearly the same magnifying powers should be employed by all the observers.

(3.) That attention should be directed to observations of real internal contacts as the chief points.

Phase Fig. (5).

(4.) That apparent internal contacts should also be most carefully observed.

Phase Fig. (6).

5. That in observing apparent internal contacts two times should be given corresponding to the "Contactus

dubius" and "Contactus certus" of Hell and Sajnovics, and the mean taken for the time apparent contact.

6. That diagrams or drawings should in all cases be furnished by the observers to represent, as nearly as possible, the appearance of the planet corresponding to each recorded time of observation. In making such drawings the essential point should be to represent accurately the comparative breadths of the ligament with respect to the diameter of *Venus*. It must be remembered that such words as "instantaneous" refer only to the observer.

I lay more stress upon the observation of apparent internal contacts, because I believe that under unfavourable atmospheric circumstances, the real internal contacts would not be generally caught by the observers until long after the contact had been established.

Observation of Transit of Mercury, end of, on November 4, 1868. By Professor C. Piazzi Smyth.

Telescope aperture = 6·4 in.; magnifying power = 80.

Definition very bad, from undulations in the atmosphere sometimes so excessive as to cause the planet, otherwise signally black and round, almost to disappear; and making its circumference and the Sun's limb always to consist, more or less, of broken ripplings of light and shade, completely preventing thereby any real phenomenon of the expected "*rupture of the luminous thread*" at internal contact.

	G.M.T. h	m	s
Internal contact, thus unfavourably observed, at	21	0	7
External contact, or planet completely off the Sun's disk, at ..	21	2	20

Royal Observatory, Edinburgh,
November 12, 1868.

The Transit of Mercury, November 4, 1868, observed at the Observatory, Durham. By J. J. Plummer.

(*Communicated by Prof. Chevallier.*)

The transit of *Mercury* was observed at Durham under exceptionally favourable circumstances; the Sun rose in a perfectly cloudless sky, and not a cloud was seen during the whole course of the observations. The limb of the Sun was, however, very

tremulous, and the planet ill defined, owing to the low altitude; but both improved very much towards the time of egress.

The instrument used was the Fraunhofer Equatoreal of the Observatory. Aperture, $6\frac{1}{2}$ inches, and 8 feet 3 inches focal length, furnished with a wire-micrometer; power 112.

The times of the internal and external contacts at the egress were observed as follows:—

	Durham S.T.	Green. M.T.
	h m s	h m s
Internal Contact	11 53 21·0	21 0 12·0
External Contact	11 55 38·5	21 2 29·2

The first time is considered quite exact; the second, owing to the undulations of the Sun's limb, is less certain.

Some measures were made of the differences of right ascension of the Sun and planet; they have been corrected for refraction, parallax, and for the motion of the Sun; and are compared with the Elements given in the *Nautical Almanac.*

Durham Sid. T.	Green. M.T.	No. of Wires obser^{d}.	Limb of ☿ obser^{d}.	Difference of R.A. of Centres, ☿ − ⊙	Computed from N.A.	C − O.
h m s	h m s			s	s	s
11 1 42·9	20 8 42·0	3	I.	−42·338	−42·432	−0·094
11 9 42·7	20 16 41·2	3	II.	−45·188	−45·354	−0·166
11 15 21·7	20 22 19·0	3	II.	−47·442	−47·414	+0·028
11 34 1·2	20 40 55·5	2	I.	−54·044	−54·222	−0·178
11 37 31·1	20 44 24·7	3	I.	−55·267	−55·498	−0·231

The following differences of declination were also observed and are similarly corrected and compared with the *Nautical Almanac*:—

Durham Sid. T.	Green. M.T.	No. of Measures.	Limb observed. ☿ ⊙	Difference of Decl. of Centres.	Computed from N.A.	C − O.
h m s	h m s			′ ″	′ ″	″
11 25 41·2	20 32 36·7	3	N.L.−S.L.	−7 41·11	−7 37·46	+3·65
11 27 26·5	20 34 21·8	2	S.L.−S.L.	−7 35·73	−7 32·94	+2·79

An attempt was also made to measure the diameter of *Mercury.* The micrometer employed was a double image micrometer of Mr. Airy's original construction. The value of its screw was determined in March 1868, and found to be 18″·693, a value closely agreeing with that determined some years ago by the Rev. R. Thomson. The mean of eight measures gives for the diameter 9″·001, which is a somewhat small result. Owing to the rapidity with which the observations were necessarily taken, and the rather unsatisfactory image of the planet, this result cannot be considered of any great value; the individual measures are, however, very fairly accordant.

On the Transit of Mercury. By W. Lassell, Esq.

1868, Nov. 4, $20\frac{1}{2}$ hours. Observed part of the transit of *Mercury*. Telescope used, a Gregorian of 4·7 inches aperture, power 120. The Sun not visible over the trees until about $19^h\ 50^m$, and then too much disturbed to see anything well. At $20^h\ 20^m$ undulation less, but still the planet and Sun's limb greatly agitated. At $20^h\ 55^m$ settled myself at the telescope to observe last contacts, though from the state of the limbs no *accurate* observation could be expected. The nearest times which it was possible to note were the following:—

	h	m	s
First contact	21	0	12
Planet apparently bisected at	21	1	12
Last contact, best observed of the three ..	21	2	22·8

Times by watch, which was slow for G.M.T. 0 0 25

Assumed places of Ray House, Latitude 51° 31′ 23″

Longitude $2^m\ 49^s{\cdot}3$ west.

Some fine spots were on the Sun, one towards the eastern limb, almost exactly the same size as the planet; but the contrast of shade was so striking as to give an optical proof how very far even the central part of a solar spot is from absolute blackness.

Ray Lodge, Maidenhead,
November 5th, 1868.

On the Transit of Mercury. By F. C. Penrose, Esq.

Thinking that, amongst the numerous and far more important communications which you will receive of the observations of the transit of *Mercury*, there may yet be some value for one made with a small aperture and moderate magnifying power — the value, if any, being to illustrate the question of the transit of *Venus* in 1769 — I beg to communicate the following observation:—

Focal length	30in.
Aperture	$2\frac{1}{4}$

Concave single lens eye-piece; magnifying power 70.

I do not doubt that my G.M.T. would be true to half a second. The error could not exceed a second.

The state of the definition fairly good with the power employed, but would not have suited a higher. The Sun's limb boiled all along, but not very violently.

	G.M.T. h m s
Thread of light interrupted at internal contact	21 0 53·7
Boiling recommenced along the limb after the planet, and 10s after the notch ceased to be visible	21 2 37·9

Both observations appeared very sharp and sudden, but the last observation was the best.

The time which elapsed between these observations was

$$1^m\ 44^s{\cdot}2$$

the calculated duration appears to be

$$2^m\ 34^s{\cdot}8$$

and the time of final egress for this station

$$21^h\ 2^m\ 57^s{\cdot}7.$$

It appears, therefore, that irradiation and other circumstances connected with the smallness of the telescope and low power employed, reduced my observed duration by $50^s{\cdot}6$; and this effect was more considerable at internal than at external contact. And this nearer coincidence in the latter case may, perhaps, be accounted for by the diminution of the disturbing effect of the atmosphere on the Sun's rays in the line of vision, by the intervention of the planet.

Wimbledon, Nov. 11, 1868.

On the Transit of Mercury. By James Buckingham, Esq.

On the morning of November 5th, 1868, excellent observations were made on *Mercury* during his transit over the Sun's disk, as from the time of sunrise till the end of the transit there was no trace of a cloud near, but a fine clear sky, and, about half-an-hour after the Sun had risen, the air became very steady. The contacts were both seen on the preceding limb of the Sun, and noted with great exactness by the chronograph at the following times, viz. :—

	h m s	
Internal contact at	11 59 8·61	Sidereal local time.
External contact at	12 1 59·09	,, ,,

Or in Greenwich mean time :—

	h m s
	21 0 18·77
	21 2 40·85
Duration	0 2 22·08

The internal contact occurred very suddenly by an apparent elongation of the planet's semi-diameter near the place of contact, when, by the previous appearance of the streak of light of the Sun, I had expected some ten to twelve seconds would have elapsed before the contact would occur; but at the time noted the circularity of *Mercury* was lost at point of contact (when all light of the sun at that part disappeared) as roughly shown in the sketch.

During the transit the image of the planet was beautifully defined and round, absolutely *black* without the slightest *penumbra*. During my observations, twice there existed an unsteadiness, and I noticed the image of *Mercury* expand and contract rapidly; but, during a long steady period, I obtained ten sets of measures of the planet's diameter, with a power of 340 on my 9-inch Equatoreal; the mean of them gave $7''{\cdot}651$, differing $2\frac{1}{4}''$ from that given in the *Nautical Almanac;* I used the micrometer (parallel wire) on my larger instrument, power 610; the result of five sets of measures differed only 0·3 from the previous (more or less, viz. $7'{\cdot}618$), and I am quite satisfied these measures are approximately true, but the great discrepancy for some moments very much surprised me. Yet, on reflection, I think it quite consistent that the observed diameter of a planet in transit should appear less than the true, owing to the effect of irradiation from the intense light of the Sun appearing to encroach on the dark body; and this supposition, I apprehend, is consistent, because so soon as the actual limb of the planet had really covered the true limb of the Sun all irradiation existing up to that moment (and thereby reducing the apparent diameter of the planet) would cease, and the *true semi-diameter of the planet* would be apparent and cause the appearance of elongation as described.

	°	′	″	
Observatory	51	29	13	N. Lat.
	0	4	58	W. Long.

I have thought it possible these few remarks may interest the Society, and beg to submit them.

Westmoreland House, Walworth Common.

On the Transit of Mercury. By C. L. Prince, Esq.

The weather this morning was favourable for observing the latter part of the transit of *Mercury* over the Sun's disk.

The definition was unusually bad for some time after sunrise, but at eight o'clock it much improved, and the planet appeared as a circular black spot on the Sun's disk. Its colour was much more intense than the nuclei of the solar spots which were present. I observed nothing worthy of note till the planet was

approaching internal contact, when a sudden elongation or lengthening out of the planet's horizontal diameter, almost to a point, occurred; and internal contact appeared to happen sooner than might have been expected from the previously circular form of the planet. This distortion of the figure of the planet continued till its centre was upon the Sun's limb; when it ceased. During the remainder of the egress, even to the moment of last contact, no distortion whatever occurred. The telescope I employed was my Equatoreal of 6·8 inches aperture and 12 feet focal length. The magnifying power was 210 in a Dawes solar eyepiece, with blue glass shade. The times of contact were as follows:—

	h	m	s	
The Internal Contact took place at	9	0	54·1	Local Mean Time.
The External ,, ,,	9	3	11·6	,,

My Observatory, in time, is 24″ East of Greenwich.

Uckfield Observatory, Nov. 9, 1868.

On the Transit of Mercury. By G. Williams, Esq.

Having been more fortunate than some of my neighbours in seeing the recent transit of *Mercury*, I venture to send a drawing and some measures I obtained of the planet's position on the Sun's disk.

A parallel wire micrometer was made use of, the Sun's limb being made to run along the upper moveable wire; the difference in time of transit between the Sun's preceding limb and the planet over the fixed wire was then taken; the time at once noted and the lower moveable wire brought up as quickly as possible to the planet's position.

The revolutions of the micrometer-screw were reduced to seconds in the usual way, and the difference in the times of transit converted into seconds of arc, and corrected by the cosine of the declination.

I once fancied I saw a ring of ruddy coloured light round the planet, but, for the greater part of the transit, nothing of the kind was visible.

There was no apparent elongation, pear shape, or black drop at the egress of the planet; but the boiling of the limb, which was considerable, may account for the absence of these appearances.

The telescope used was the equatoreal refractor, 4·25 inches aperture and 63 inches focus, by Messrs. Cooke and Sons; powers 71 to 140.

2 *Devonshire Road, Prince's Park, Liverpool,*
Nov. 11, 1868.

The figure which accompanies the foregoing Note shows the following three positions of the planet in regard to the Sun's disk, viz.—

At	20 h 15 m 54 s	$x = 288''\cdot67$	$y = 474''\cdot27$
	20 26 56	259·81	496·90
	20 54 26	121·24	570·88

where x is the distance in the direction of a parallel of declination, and y the distance in the direction at right angles to this, from an origin outside the Sun's disk, such that in regard to it the co-ordinates of the Sun's centre are

$$x = y = \text{Sun's radius} = \tfrac{1}{2} \cdot 1941''\cdot4;$$

and further, that at egress the times of internal contact and of external contact were $21^h\ 0^m\ 18^s$ and $21^h\ 2^m\ 26^s$ respectively.—Ed.

On the Transit of Mercury. By Capt. Noble.

In common, I suppose, with every possessor of a telescope in England, I observed the transit of *Mercury* on the morning of Thursday, November the 5th.

Most unfortunately, through a circumstance over which I had no control, I had to withdraw my eye from the instrument at the instant of last internal contact; but, on re-applying it to the telescope immediately after this had taken place, I then noticed that the planet seemed to be somewhat drawn out, or *squared*, as roughly shown in my sketch; the double inversion of which arises from my observation having been made with the "Hodgson" reflector. I watched *Mercury* across the Sun's disk with a power of 74, but exchanged this for one of 154, after internal contact had taken place, and with this observed the egress. The last external contact or final emersion occurred at

$12^h\ 2^m\ 30^s$ L.S.T. = Nov. 4, $21^h\ 3^m\ 0^s\cdot99$ L.M.T.

a determination which I scarcely think is 1 second ± in error. I employed, as usual, my 4·2-inch Equatoreal of 61 inches focal length in making the observation.

Forest Lodge, Maresfield, Sussex,
12 *Nov.* 1868.

The transit of *Mercury* was observed at Rome by the Rev. Father Secchi, thus:—

1868, Nov. 5, Rome M.T.

	h	m	s
Ring broken at	21	50	5·91
Planet out at	21	52	27·46

Four Photographs of the Sun and *Mercury*, taken at Ely on November 5, communicated by the Rev. Prof. Selwyn, were exhibited at the Meeting. Prof. Selwyn writes:—

"At the taking of the third of the series, the planet was close to the edge, and was visible on the negative; but I cannot be quite sure of his appearance in the print. We ought to have given a larger aperture, or longer exposure, to insure a good representation of *Mercury* in the comparative darkness of the Sun's limb, but it was not thought of. The fourth is given, at a later hour, to remove all doubt as to the nature of the round spot representing *Mercury*."

On the Appearance of Mercury at its Transit, November 5, 1868. By William Huggins, F.R.S.

As buildings intercepted the view of the Sun from my observatory before $8^h\ 30^m$, I determined not to attempt any measures of *Mercury*, but to devote the short time at my disposal to a careful scrutiny of the appearance which the planet might present when seen upon the Sun.

The Sun's edge was a little tremulous from atmospheric agitation, but the solar surface was so well defined that the bright granules of which it is composed could be distinctly seen. The planet appeared as a well-defined, round, black spot. Whilst carefully examining the immediate neighbourhood of the spot for the possible detection of a satellite, I perceived that the planet was surrounded with an aureola of light, a little brighter than the solar disc. The breadth of the luminous annulus was about one-third of the planet's apparent diameter. The aureola did not fade off at the outer margin, but remained of about the same brightness throughout, with a defined boundary. The aureola was not sensibly coloured, and was only to be distinguished from the solar surface by a very small increase of brilliancy.

Almost at the same moment that I first perceived the surrounding annulus of light, I noticed a point of light nearly in the

centre of the planet. This spot of light had no sensible diameter with the powers employed, but appeared as a luminous point.

These phenomena, the aureola and the spot of light on the planet, were distinctly visible as long as the transit continued.

The whole aperture, 8 inches, of the telescope was in use, with a prismatic solar eye-piece, and powers of 120 diameters and 220 diameters. A sliding wedge of neutral tint glass allowed the apparent brightness of the Sun to be rapidly varied, but with all parts of the wedge brought before the eye, the phenomena described above continued to be visible.

It is of importance to state that the aureola was seen most plainly when the darkest part of the wedge was before the eye. This observation is scarcely what would have been expected on the supposition that the aureola is merely an optical or ocular phenomenon, for the conditions when the Sun was greatly darkened were favourable for the discrimination by the eye of the existence of a small difference of illumination; but they were, in the same degree, unfavourable for a mere optical effect produced by contrast. I am sorry that I omitted to notice if the definition of the granular structure of the solar surface was equally good, immediately round the planet where the aureola appeared.

I watched carefully the bright point upon the planet as *Mercury* was passing off the Sun, to ascertain if this luminous spot could be seen after the planet had become invisible. I kept it steadily in view until the part of the planetary disc, where the point of light was situated, reached the Sun's limb, I then ceased to see it. This observation must be regarded as negative merely, and not as proving the invisibility of the luminous point when the planet had passed off the Sun, for so small a point of light might be easily overlooked when the form of the planet was no longer visible to serve as a guide to the eye.

It may be well to state that at the time of making these observations, the appearances described by Schröter, Moll, and others, which I had not read for some years, were absent from my memory. I was not looking for these appearances, and it was some little time before I would believe in their reality.

If *Mercury* be surrounded by a transparent atmosphere the solar rays would be bent in on all sides and would cross in front of the planet, but would then proceed in directions far too much removed from the line joining the Sun, *Mercury*, and the Earth, to be received by the telescope. Such an atmosphere, in consequence of its power of turning aside the solar beams incident upon it, should appear darker than the solar surface.

Similar phenomena have been observed at some former transits. A sort of ring of faint light was seen by Plantade at the transit of 1736; also by Proserpin; also by Flaugergues in 1786, and in 1789, and 1799. He calls it "un anneau lumineux." Mechain, Messier, Fritsch and Seyffler observed a similar phenomenon. It is also described by Schröter and Harding during the transit of 1799. In 1832 Dr. Moll saw it as "a nebulous ring of

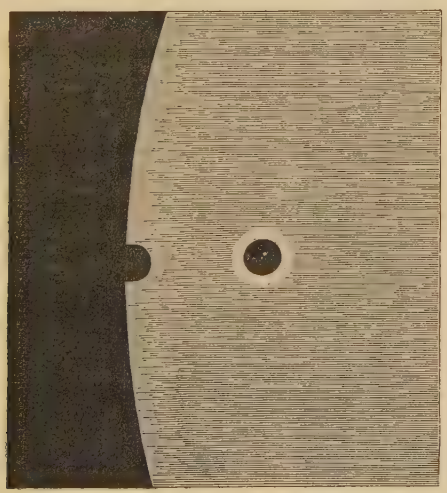
a darker tinge approaching to a violet colour." Some of these observers appear to have considered the aureola to be slightly brighter, and others as in a small degree darker than the Sun.

The other phenomenon, that of a bright spot on the planet, has been seen at former transits. At the same time that Schröter, in 1799, perceived the ring, he saw on the body of the planet a small bright spot, which was seen independently by Harding. At a later period of the day, Schröter saw the luminous speck, sometimes on one part of the disc and sometimes on another, being unable to keep it constantly in view.

A bright spot, not in the centre of the disc, was seen by Dr Moll at the transit of 1832.*

Professor Powell has proposed to account for the bright spot as a phenomenon depending on the diffraction of light.† As, however, in all the observations the spot was not seen in the centre of the planet's disc, this explanation seems to be scarcely applicable.

The following appearance was noticed almost immediately after the planet's disc came up to the Sun's limb. The spot appeared distorted, spreading out to fill up partly the bright cusps of the Sun's surface between the planet's disc and the sun's limb. This appearance increased as the planet went off the Sun, until when the disc of the planet had passed by about one-third of its diameter, it presented the form represented in the diagram, in which the



* See Dr. G. Moll on Transit of 1832, *Mem. R. Astron. Society*, vol. vi. p. 116; also, Breen, *The Planetary Worlds*, p. 138. I have been unable to refer to Schröter's original observations in the *Hermographische Fragmente*. This work is not in the library of the R. Astronomical Society, nor in that of the Royal Society.

† Prof. Powell on Irradiation: *Mem. R. Astron. Society*, vol. xviii. p. 69.

margin of the disc from points at the end of a diameter parallel to the Sun's limb, instead of continuing its proper curve, appeared to go in straight lines up to the limb, thus entirely obliterating the cusps of light, which would otherwise have been seen between the planet and the limb. In the diagram the aureola and the bright spot are not repeated in the figure of the planet on the Sun's limb.

A Determination of the Constant of Nutation from the Observations of Polaris in R.A. made at the Royal Observatory, Greenwich, from 1851–1867. By E. J. Stone, Esq.

In the June number of the *Monthly Notices* of the Royal Astronomical Society appears an abstract of a paper of mine on "A Determination of the Constant of Nutation from the Observations in N.P.D. of *Polaris*, *Cephei* 51, and δ *Ursæ Minoris*, made with the Transit Circle of the Royal Observatory, Greenwich, 1851–1865." In the present paper I have discussed the observations of *Polaris* in R.A. made with the Transit Circle of the Greenwich Observatory, from 1851 to 1867, both inclusive.

The observations, as extracted from the yearly Catalogues, are given in Table I.

Mean Right Ascensions of *Polaris* for Jan. 1, extracted from the Annual Catalogues.

Year.	Fraction of Year.	No. of Obs.	R.A. h m s
1851	0·46	99	1 5 19·02
1852	0·43	128	1 5 36·75
1853	0·61	76	1 5 54·65
1854	0·53	169	1 6 12·98
1855	0·60	110	1 6 30·86
1856	0·48	113	1 6 49·36
1857	0·52	150	1 7 7·37
1858	0·47	191	1 7 25·90
1859	0·55	155	1 7 44·23
1860	0·46	92	1 8 3·17
1861	0·50	116	1 8 21·50
1862	0·59	67	1 8 40·52
1863	0·48	78	1 8 59·63
1864	0·47	112	1 9 19·08
1865	0·52	110	1 9 38·06
1866	0·53	93	1 9 57·87
1867	0·64	77	1 10 17·34

The clock-errors from 1851 to 1855 were determined from the right ascensions of fundamental Stars, as given in the Greenwich

twelve-year Catalogue: those from 1856 to 1861 from the R.A.'s of the Greenwich six-year Catalogue: those from 1862 to 1867 from the R.A.'s of the Greenwich seven-year Catalogue. To bring the system to uniformity, the R.A.'s from 1851 to 1855 will be increased by $+0^{s}\cdot013$: those from 1856 to 1861 by $+0\cdot004$.

The Nutation constant adopted during the years 1851–1856 was $9''\cdot25$. The constant from 1857 to 1867 was $9''\cdot224$. The following corrections will be applied for the effect of this difference of adopted constants:—

	s
To the Results of the Year 1851	+0·018
1852	−0·004
1853	−0·029
1854	−0·045
1855	−0·060
1856	−0·066

The following are the corrections applied to the results of each year to allow for the precessional motion in R.A. from the year 1859. The Pulkova constants have been used:—

	m s
Correction to Result for 1851	= 2 24·397
1852	= 2 6·766
1853	= 1 49·018
1854	= 1 31·151
1855	= 1 13·165
1856	= 0 55·058
1857	= 0 36·829
1858	= 0 18·477
1859	= 0 0·000
1860	= 0 18·604
1861	= 0 37·336
1862	= 0 56·195
1863	= 1 15·184
1864	= 1 34·306
1865	= 1 53·561
1866	= 2 12·951
1867	= 2 32·477

Then, assuming

Proper Motion in R.A.	$= 0^{s}\cdot1148 + p$
True Constant of Nutation	$= 9''\cdot224 + y$
Mean R.A. of Polaris, 1859, Jan. 1	$= 1^{h}\,7^{m}\,44^{s} + x$

we have the following equations of condition for the determination of x and y:—

				w.
1851	x	$+ 0{\cdot}680\, y =$	$0{\cdot}366 + 8\, p$	99
1852	x	$- 0{\cdot}141\, y =$	$0{\cdot}329 + 7\, p$	128
1853	x	$- 1{\cdot}119\, y =$	$0{\cdot}341 + 6\, p$	76
1854	x	$- 1{\cdot}720\, y =$	$0{\cdot}673 + 5\, p$	169
1855	x	$- 2{\cdot}315\, y =$	$0{\cdot}437 + 4\, p$	110
1856	x	$- 2{\cdot}543\, y =$	$0{\cdot}700 + 3\, p$	113
1857	x	$- 2{\cdot}523\, y =$	$0{\cdot}433 + 2\, p$	150
1858	x	$- 2{\cdot}232\, y =$	$0{\cdot}496 + p$	191
1859	x	$- 1{\cdot}623\, y =$	$0{\cdot}234$	155
1860	x	$- 0{\cdot}935\, y =$	$0{\cdot}455 - p$	92
1861	x	$- 0{\cdot}035\, y =$	$-0{\cdot}062 - 2\, p$	116
1862	x	$+ 0{\cdot}920\, y =$	$-0{\cdot}019 - 3\, p$	67
1863	x	$+ 1\,608\, y =$	$-0{\cdot}013 - 4\, p$	78
1864	x	$+ 2{\cdot}212\, p =$	$+0{\cdot}200 - 5\, p$	112
1865	x	$+ 2{\cdot}587\, y =$	$-0{\cdot}190 - 6\, p$	110
1866	x	$+ 2{\cdot}652\, y =$	$+0{\cdot}115 - 7\, p$	93
1867	x	$+ 2{\cdot}369\, y =$	$-0{\cdot}055 - 8\, p$	77

Multiplying each equation by the corresponding value of $\sqrt{w}$, and reducing the equations by the method of least squares, we have for the determination of x and y the equations —

$$1936\ x - 866{\cdot}518\, y = 581{\cdot}466 + 935\ p$$
$$-866{\cdot}518\, x + 6986{\cdot}703\, y = -980{\cdot}016 - 11340{\cdot}24\, p$$

From which

$$x = +0{\cdot}252 - 0{\cdot}26\,.\,p$$
$$y = -0{\cdot}109 - 1{\cdot}66\,.\,p$$

or Mean R.A. of *Polaris*, 1859, Jan. 1,

$$= 1^{h}\ 7^{m}\ 44^{s}{\cdot}252 - 0{\cdot}26\, p$$

$$\text{Constant of Nutation} = 9''{\cdot}115 - 1{\cdot}66\, p$$

If we adopt for all the Proper Motions the values given in "Gould's Standard Places of Fundamental Stars, *second edition*," we have for

Proper Motion in R.A. of Polaris	$= 0{\cdot}^{s}1148 + p$
N.P.D. of Polaris	$= -0{\cdot}004 + \delta p$
N.P.D. of Cephei 51	$= +0{\cdot}048 + \delta c$
N.P.D. of δ Ursæ Minoris	$= -0{\cdot}043 + \delta m$

The following will then be the resulting values of the constant of Nutation : —

(1) $N = 9''\cdot115 - 1\cdot66\ p$ from the R.A. of Polaris
(2) $N = 9\cdot109 + 6\cdot1\ \delta p$,, N.P.D. of Polaris
(3) $N = 9\cdot143 - 3\cdot6\ \delta c$,, N.P.D. of Cephei 51
(4) $N = 9\cdot214 + 1\cdot75\ \delta m$,, N.P.D. of δ Ursa Minoris.

Giving (1) and (2) twice the weights of (3) and (4), we have for the final value of the constant of Nutation,

$$9''\cdot134 - 0\cdot55\ p + 2\cdot02\ \delta p + 0\cdot29\ \delta m - 0\cdot6\ \delta c.$$

Occultation of Stars by the Moon, 8 Sept. 1868.
By J. Joynson, Esq.

		h	m	s	
71 Tauri	Dis.	11	31	17·7	G.M.T.
θ^2 Tauri	Dis.	12	40	16·5	
	Re.	13	41	25·5	
θ^1 Tauri	Dis.	12	46	15·4	
	Re.	13	36	7·1	
81 Tauri	Dis.	13	45	6·8	
	Re.	14	18	8·6	
1391 B.A.C.	Dis.	14	10	21·8	
85 Tauri	Re.	14	57	20·7	

The occultation of α *Tauri* could not be observed owing to dense cloud.

Waterloo, near Liverpool.

Mr. Chambers has communicated the following Errata in his "Catalogue of Binary Stars," *Monthly Notices*, vol. xxviii. pp. 174–179.

Part I.

The columns of Epoch should be headed 1800 +, *not* 1880 +
No. 4. 34 *Andromedæ* should be 36, and the Mag. of A, 6, *not* 4.
No. 31. The Epoch should be '63·1, *not* '66·1.
No. 80. Struve's synonym should be 3127, *not* 2137.

Part II.

No. 19. The Epoch should be '64·7, *not* '65·7.
No. 31 is misplaced in R.A.; it should follow 28.
No. 42. The Decl. should be 21° 36′·3, *not* 21° 26′·3.
No. 43. The Magnitude of B should be $7\frac{3}{4}$, *not* $8\frac{3}{4}$.

Bickley, Kent, August 6th, 1868.

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